

AMERICAN NEUROLOGICAL ASSOCIATION.

SEVENTH ANNUAL MEETING.

(OFFICIALLY REPORTED BY M. JOSIAH ROBERTS, M.D.)

First day, afternoon session.

The AMERICAN NEUROLOGICAL ASSOCIATION convened in the New York Academy of Medicine, June 15, 1881, for its seventh annual meeting. In the absence of Dr. Miles, the retiring President, the Secretary, Dr. Seguin, called the Association to order at 2.30 P.M., and introduced the President-elect, Dr. Roberts Bartholow, of Philadelphia.

Present—Drs. Amidon, Beard, Bartholow, Birdsall, Hammond, W. A., Hammond, G. M., Jewell, McBride, Morton, Ott, Rockwell, Seguin, Shaw, Spitzka.

Dr. BARTHLOW remarked that upon his arrival in New York he had been informed of the customary practice of the incoming President of the Association to make a few introductory remarks; but as this information was a surprise to him, he would take the liberty of proving an exception to the rule.

The reading of the minutes of the last annual meeting being called for, it was moved by Dr. McBride that as they had been printed and sent to each member for perusal their reading should be dispensed with. Carried.

The Council and the Secretary had no reports to make.

The Treasurer, Dr. E. C. Seguin, of New York, read his report, which, upon motion of Dr. Shaw, was adopted.

NOMINATION OF CANDIDATES.

Dr. J. S. JEWELL, of Chicago, nominated Drs. S. V. Clevenger and H. Gradle, of Chicago, for active membership.

The Secretary read a note from Dr. E. C. Spitzka, nominating Dr. Burt G. Wilder, of Ithaca, New York, for active membership.

Dr. E. C. SEGUIN nominated Drs. Charles K. Mills and Wharton Sinkler, of Philadelphia, for active membership.

The above nominations were referred to the Council to report at a future session.

COMMITTEE ON NOMINATION OF OFFICERS.

The President appointed the following Committee on Nominations: Drs. Jewell, of Chicago; Isaac Ott, of Pennsylvania; Amidon, Morton, and Rockwell, of New York.

MISCELLANEOUS BUSINESS.

Dr. J. SHAW moved that no case be presented to the Association unless it formed the basis of a written communication. He remarked that this motion was not intended to exclude the recitation of cases in the discussion of papers, but to cut off some irregular work which had been found unprofitable to the Association. Carried.

The Secretary read a letter from Dr. J. K. Bauduy, of St. Louis, to the effect that if his resignation as a member of the Association, which had been transmitted some months previous, had not been presented and accepted he would like to withdraw it and retain his membership. As his resignation had already been accepted, upon motion of Dr. Shaw, the matter was referred to the Council with power to act.

The Secretary read a letter from Dr. T. M. B. Cross, of New York, tendering his resignation, which was referred to the Council for action.

Dr. SEGUIN stated that he would read an amendment to the constitution, which he had proposed at the last annual meeting, and was to be acted upon at this. It was as follows:

Article III. In addition to Active Members there shall be a class of Honorary Members, not to exceed twelve in number, and a class of Associate Members not to exceed twenty-five in number.

Honorary Members shall be nominated in writing by six Active Members, reported upon by the Council, and elected only by an unanimous vote of the members present at the session following the one at which the nomination is made.

Associate Members shall be nominated in writing by two Active Members of the Association, reported upon by the Council, and

elected by a majority of the members present at the session next following the nomination.

Dr. JEWELL moved that the amendment be adopted. Seconded.

Dr. SEGUIN remarked that there were at present no Honorary or Associate Members of the Association. The reason why no step had been taken to secure the same was that the original promoters of the Association thought it best to wait until it had made some substantial progress in order to avoid the appearance of asking for names merely for the purpose of giving the organization a standing. Carried unanimously.

There being no further miscellaneous business, Dr. J. C. Shaw was called upon to read his paper, entitled "Tendon reflex in general paralysis of the insane."

At the meeting of the Association in 1879 he had presented a paper on the "tendon reflex in the insane." The opinions then expressed had been confirmed by subsequent extended observations, and many new facts had been learned. The object of his present paper was to communicate these to the Association. Observations had been made on 70 cases of general paralysis in men, and 10 cases in women. In the men it was found that the reflex was normal in 28 cases, that it was slight in 8 cases, absent in 13 cases, and exaggerated in 21 cases. Of these, *post-mortem* examinations had been made in 18 cases, and the spinal cords studied microscopically after hardening and mounting. A brief history was attached to each of these cases, and the state of the reflex as observed during life. In 5 of the cases the reflex was found absent, and *post-mortem* examinations showed sclerosis of the posterior column. In 4 cases the reflex was found normal during life, and *post-mortem* examination of the cords showed no lesion. In 8 cases the reflex was found exaggerated, and the *post-mortem* revealed sclerosis in the lateral columns on both sides, with a certain amount of diffuse sclerosis in all parts of the cords. Observation had shown that this exaggerated reflex is in direct correspondence with marked difficulties in speech and hemiparetic attacks, and this connection was susceptible of an anatomical demonstration. The doctor announced his intention of making another communication upon this subject.

Remarks.

Dr. JEWELL inquired for Dr. Shaw's idea of the inner mechanism in the cases cited, in which there was abolition of the tendon

reflex accompanied with disease of the posterior columns or portions of the same ; remarking at the same time that an answer to his question might involve Dr. Shaw's idea of the function of that part of the cord.

Dr. SHAW said that he believed the disease of the posterior column interfered with conduction in the sensory parts, and in that way the reflex was abolished. Whenever he had found the reflex abolished, *post-mortem* examination had shown the posterior columns to be very much diseased.

Dr. ROCKWELL thought it would be interesting to know how much experience there had been in observing cases where the tendon reflex was absent and the cord healthy. He had observed two cases in which there was no tendon reflex, and in which, so far as he was able to determine, the cord was perfectly healthy.

Dr. SHAW said that such cases had been observed, and that he himself had seen one. Examples of this sort were exceptional, and he could give no explanation of them. He was of the opinion that Westphal thought there was in such cases some disease of the cord, though it could not be demonstrated.

The President inquired if it was a true reflex.

Dr. SHAW believed that it was now claimed to be a true reflex. There had been several points observed which were not clear to him. He had observed all the facts he could in hopes of ultimately arriving at a rational explanation of them.

Dr. SEGUIN remarked that Dr. Gray had read a paper before the Association some three or four years ago upon the frequency of tendon reflex in healthy persons. He had examined a number of students in the Long Island College Hospital, and reported several examples of absence of the tendon reflex in healthy individuals. Dr. Seguin was much interested in Dr. Shaw's paper, and thought his investigations into the pathology of paralysis in the insane might lead to a fundamental pathological classification of general paralysis. The cases he had seen he had been only able to define in a coarse clinical way, owing to the want of a pathological basis for an useful and intelligent classification.

Dr. JEWELL remarked that he had been much interested in the subject of tendon reflex for a number of years, and the conclusion which he had at present reached was as follows : Where he found it absent he always suspected, unless there was good reason to the contrary, disease of the posterior columns of the spinal cord. The mechanism of such cases was exceedingly simple. The diseased parts lay in the path of the ingoing impulses, which have to

pass through the spinal cord. If there are no signs of disturbance besides the absence of the reflex he regarded it as one of a class of cases, of which he himself was an example, in which the tendon reflex was absent. Some persons were sensitive in this way, others hardly at all. The absence of this sign did not make it necessary for us to suppose that there is disease of the spinal cord unless there was some unequivocal sign of disease of the sensory tract. He thought the matter of absent and exaggerated reflexes had been made to appear more singular than it really is. He concurred with Dr. Seguin as to the great value of Dr. Shaw's paper, for it brought the results of *post-mortem* examination face to face with symptoms observed during life, and he felt sure that the author of the paper was on the right road, though a very long road, to a solid nerve pathology.

There being no further discussion, the Secretary proceeded to read the paper of Dr. John J. Mason, entitled, "Notes on the central nervous system of reptiles."

The paper consisted first, of observations on a lateral fibrous cord in ophidian saurians, the homologue of the ligamentum denticulatum; second, of observations on a normal ventro-dorsal compression of the myelon at the acute angle made by the articulation of the last cervical vertebra with the carapax in *Cistuda Carolina* (Holbrook), box turtle; third, of observations on the optic chiasm of the *Anolius Carolinensis*—American Chameleon.

Remarks.

Dr. SPITZKA remarked that there was room for the study of other peculiarities of the appendages of the nervous system which, without any presumable physiological value, had some interest as morphological curiosities. He himself had observed in three marine turtles that there was a thin rod of cartilage running from the dorsal face of the baso-occipital bone to the ventral face of the myelancephalon; whether this is connected with the nervous axis directly, or fused with its membranous investments, he had not ascertained. It certainly appeared to be a very aberrant anatomical feature.

The next paper was by Dr. Isaac Ott, "The action of an irritant."

The paper consisted of a recitation of experiments and the conclusions reached were as follows:

1. Certain irritants applied to the skin produce a variety of phenomena of the nervous system ; other irritants do not.
2. These phenomena are not due to circulatory changes, as usually held, but to an excitation of the central nervous system.
3. Irritations of the skin diminish the irritability or partially palsy the motor nerves of the opposite side.
4. They also produce anæsthesia by a stimulation of inhibitory ganglia.
5. When applied to the nose they inhibit the heart and respiratory centres.
6. They excite the monarchical vaso-motor centre.
7. They lower the temperature.
8. They dilate the pupil.

Remarks.

Dr. SPITZKA thought it was a defensible statement to claim that, spontaneous exciting action resided in no special centre ; he thought that there was no central action that could not be traced back to a starting-point.

Dr. JEWELL inquired if Dr. Ott was of the opinion that these inhibitory centres were to be found in the base of the brain and that they could be reinforced both from centres above and below.

Dr. OTT replied in the affirmative.

Dr. JEWELL held a loose opinion to the effect that the reinforcing centres could not be looked upon as subordinate but must always be looked upon as super-ordinate or at least coördinate ; this was only an opinion and he would not undertake to defend it. He thought that the reinforcing centres were never below. Parts below could excite those above but not charge them with force. He did not question the facts brought forward by Dr. Ott, but only their explanation ; he thought that excitation might go upward or downward from the basal parts of the brain and in this way launch nerve excitations upon the central nerve axis, but reinforcing centres must be above not below those reinforced.

Dr. SEGUIN remarked that it would be interesting in this connection to recall Brown-Séquard's experiments with irritating vapors. He (Brown-Séquard) thought it was possible to arrest severe headache by forcing carbonic acid gas into the nostrils. Dr. Seguin had seen him arrest epileptic fits in guinea-pigs by forcing carbonic acid into their throats under pressure. A quack remedy for the cure of epilepsy, used in France some thirty years ago, was

the application of ammonia to the pharynx by means of a swab. He believed one of the German physicians interested in neurology had suggested the swallowing of a large mouthful of salt at the time of aura.

Dr. MORTON remarked that in two or three instances he had observed curious facts that could only be explained in this way. One patient had a tonic spasm involving most of the muscles of the face. This spasm would come on and last for several hours and then pass off. He made the experiment frequently of applying the galvanic current to the facial nerve, which would quickly develop the spasm, and then resolving it by striking three or four sparks from the static-electrical machine. He repeated the experiment with the Faradic current and satisfied himself that the spasm was truly reflex of the motor track of the trigeminal nerve. He then tried some experiments in the treatment of mimic spasm.

There being no further discussion, the Secretary proceeded to read the paper of Dr. H. M. Bannister, bearing the title of, "A peculiar effect of the bromides on certain insane epileptics."

Dr. BANNISTER related in his paper the apparent effect of the bromides on an epileptic under his observation at the asylum at Kankakee in Illinois. After the use of the drugs in question for a week or two, the epileptic paroxysms were interrupted, but there came on gradually a state of mental irritability, which at last rose to the pitch of homicidal mania with delusions. This state had been often produced in the same patient by the same means. Upon withdrawing the bromide the maniacal violence gradually subsided, and entirely disappeared on the return of the epileptic attacks.

Dr. BANNISTER referred to other cases of which he had learned, and to the rarity of similar observations in medical literature. He thought the observation important and suggestive, but offered no definite opinion as to whether the occurrence of the mania was directly or indirectly due to the bromides.

Remarks.

Dr. SPITZKA thought Dr. Bannister was mistaken as to the absence of records of this kind. There was a German alienist by the name of Stark, who had published a very carefully written paper, in which he admitted the statement made by the author of the paper just read, and forbade the administration of the bromides to such patients. Dr. Spitzka thought the statement would

apply to 25 per cent. of the chronic epileptics in institutions for the insane.

Dr. JEWELL remarked that the alleged action of the bromides was certainly not unknown, but its importance was such as to deserve more general consideration. In the case of epileptics it was important to recognize this action of the bromides. He thought it highly probable, at least feasible, that certain of the epileptic insane were in asylums as insane persons for this very reason. Dr. Bannister had mentioned this matter to him before writing his paper, and it was chiefly on this account that Dr. Jewell had urged him to write a history of the case.

Dr. SHAW had met with this condition in the asylum, but oftener in the dispensary, especially in children who had taken large quantities of the bromide.

Dr. SEGUIN had noticed quite a number of such cases, but did not believe it was the bromides. He thought it was the suspension of the epilepsy that allowed of the excitement of the psychical centres. Yesterday he had seen a little patient, a boy of twelve years, with a singular attack of *petit-mal*. He usually had more attacks in the spring; he had many "chills." The physician of the place in which he was attending school had given him the bromides, and after taking these two or three weeks he had no chills for two months. During this time, however, he became so thoroughly unmanageable that his schoolmaster was obliged to have him return home.

Dr. HAMMOND remarked that he had not had the privilege of listening to the reading of the paper, but thought, as Dr. Seguin did, when we came to remember how frequently the bromides were given to epileptics with the effect of having the paroxysms subside, and yet did not have these symptoms develop, it was difficult to account for them, when they did occur, as being due to the bromides. In 1869 he had read the first paper, certainly the first in this country, upon bromism. In that paper he had given the history of a man who had received a blow upon the head.

He had ordered one ounce of the bromide of potassium in four ounces of water, of which a teaspoonful was to be given three times a day. The patient took the entire contents of the bottle every day. He became highly maniacal, was arrested in the street for drunkenness, and convicted of the same before a police magistrate. At the instigation of Dr. Hammond he was placed in a lunatic asylum, where he remained for two months before the effects wore off. He thought it should be known that the

bromides would kill if taken in sufficiently large doses for a long time. The effect of the bromides was apt to develop very suddenly. The best way to avert danger or relieve a patient from the effects of the bromides was through the alimentary canal by purging. He never gave more than fifteen grains three times a day.

Dr. ROCKWELL remarked that discussion bore upon a case he had under treatment, and in view of what Dr. Hammond had said, it might be that he was killing his patient. A lady epileptic patient of his had been taking the bromides four or five years with the effect of causing a cessation of the attacks for fifteen to twenty months. It was now eighteen months since she had had an attack, and she was exceedingly depressed and suffered with hysteria. The question was whether to keep on with the bromides.

Dr. HAMMOND remarked that if he had epilepsy he would take the bromides all his life, and never stop.

Upon motion of Dr. Seguin, the Association adjourned.

First day, evening session.

The Association was called to order by the President, Dr. Bartholow, at 8.30 P.M.

Present: Drs. Amidon, Bartholow, Birdsall, Gibney, Hammond, W. A., Hammond, G. M., Jewell, Kinnicutt, Morton, McBride, Rockwell, and Seguin.

The Secretary read the minutes of the afternoon session, which were approved.

REPORT OF COUNCIL.

The Council recommended Drs. S. V. Clevenger and H. Gradle of Chicago, Burt G. Wilder of Ithaca, N. Y., and Charles K. Mills of Philadelphia for active membership.

The Council also reported that the resignation of Dr. J. K. Bauduy had been rescinded, and that he was restored to active membership.

Upon motion of Dr. E. C. Spitzka, the by-laws were suspended and the Secretary was requested to cast the vote of the Association for the election of members, which was voted in the affirmative.

REPORT OF THE COMMITTEE ON NOMINATIONS.

Dr. J. S. JEWELL, chairman of the committee, reported the following nominations :

President, Dr. William A. Hammond, of New York.

Vice-president, Dr. Landon Carter Gray, of Brooklyn, N. Y.

Secretary and Treasurer, Dr. E. C. Seguin, of New York.

Councillors: Dr. J. S. Jewell, of Chicago, and Isaac Ott, of Easton, Penn.

The first paper of the evening session was by Dr. A. D. Rockwell upon "Electro-muscular contractility in infantile paralysis."

At the meeting of the Association in 1879, the author had presented a case bearing upon this point, and the history of the present case was a supplement to the first as illustrating the fact that even when the galvanic current proved utterly powerless to cause contraction of the muscles, and the paralysis is complete and the atrophy extreme, we need not, necessarily, despair of a favorable issue. The patient in the case reported was injured by forceps in delivery, so that the right arm was, from the beginning, completely paralyzed.

A number of months subsequently Dr. Rockwell found the arm atrophied, seemingly as much as it was possible for it to be, and with an entire loss of electro-muscular contractility.

No strength of galvanism elicited the slightest reaction. The case was under treatment for one month before any electrical reaction was obtained ; but from the moment this took place improvement was rapid, and the arm was now of considerable use.

In the case which he had previously presented to the Association, it was at least six weeks before galvano contractility took place. He, therefore, said, bearing this fact in mind, that persistent effort should be made for weeks, or in some cases for months, before deciding that the case was utterly hopeless.

Remarks.

Dr. SPITZKA inquired what had been Dr. Rockwell's experience in regard to improvement in the case of paralysis of central origin.

Dr. ROCKWELL replied that he did not refer to central paralysis.

Dr. HAMMOND thought that all the members of the Association would agree that it was much more difficult to restore paralysis

when due to a peripheral injury than when it was central. He had published the history of a case where the muscular contractility was entirely abolished, so far as the Faradic current was concerned, but the muscles reacted to the current from a hundred-pile voltaic battery.

He thought Dr. Rockwell's case was interesting as showing what could be done in peripheral paralysis by persistent efforts. We all knew, he said, how difficult it is to restore peripheral facial paralysis with any current that we could apply to the face. Dr. Hammond inquired the strength of current used by Dr. Rockwell.

Dr. ROCKWELL remarked that at first the strongest current would accomplish nothing, but subsequently the current from fifteen or twenty ordinary carbon cells was used.

Dr. JEWELL remarked that he was of the opinion that in many of these cases of paralysis arising from peripheral disease, where there was no evidence of traumatic destruction of nerve tissue, even though the duration of the paralysis had been long, we ought to consider them far more hopeful than they were usually considered. He remembered the case of a lady in Moline, Ill., who had been delivered by forceps, and whose left sciatic nerve, at the point where it passed through the pelvis, had been crushed by the instrument and head of the child, so that for months she was paralyzed in that member from the hip down. At the end of eleven months, the atrophy was very considerable; the limb was utterly useless. There was paralysis of motion and sensibility in all parts except where supplied by certain nerve branches in front. She was placed under treatment, and it required one or two months of careful electrical treatment before any considerable sensitiveness of skin or muscles of the member was obtained. The galvanic current was interrupted in the various ways known. At last signs of improvement began to appear, the patient began to move the limb, and finally was advised to take a trip to Europe. In accordance with his advice, she there consulted Professor Charcot, and finally a medical gentleman in Belgium, who well understood the use of electricity, and who applied it together with *massage thoroughly*. The patient was now almost entirely well. He heard from her a few days ago, and she was then able to walk up what amounted to one hundred feet of elevation without the use of a crutch or cane. Although it was customary to look upon such cases as utterly hopeless, he thought that they should no longer be considered as such, and persistent effort should be made to restore the paralyzed muscles.

There being no further discussion, the paper of Dr. H. D. Schmidt, of New Orleans, was read by the Secretary, entitled: "Destructive lesion of the left cerebral hemisphere, with general pachymeningitis, and a large hemorrhagic cyst pressing upon the right hemisphere, of thirteen years' standing."

This case of cerebral lesion was worthy of being recorded, not only on account of the extent of the lesions, but also for the long period of time through which they existed. It illustrated the ability of the brain to bear a considerable amount of injury without causing a serious disturbance of the general health, or even of the mental faculties of the patient. The paper consisted of a history of the case so far as could be obtained, and an exceedingly interesting and remarkably accurate detailed account of the pathological findings. Accompanying the paper were six admirably executed drawings representing various pathological points, to which special attention was called in the paper.

There being no remarks, Dr. J. S. Jewell, of Chicago, proceeded to read his paper advocating "The early use of strychnia in myelitis."

He said his object was to call attention to the early and free use of strychnia in subacute (diffuse) myelitis and related affections of the spinal cord, in which one of the most important conditions presumed to exist is passive congestion.

He then gave, in some detail, the histories of several cases in which the treatment by strychnia has been employed apparently with success. An oral abstract of the paper was given to save the time of the Association. He closed by a brief statement of his views as to the pathology of the cases given, and as to the mode of action of strychnia.

Remarks.

Dr. HAMMOND said that he would like to ask the author of the paper, three questions:

First, whether the drug produced any tonic spasm; second, whether the cases cited were uncomplicated cases of spinal disease; and third, why they were not cases of spinal anæmia instead of spinal congestion. He wanted to know what distinction the author made between the cases cited as those of congestion and those which were called spinal anæmia. He had met with such cases, due to liver, lung, or stomach diseases, which were cured by large doses of strychnia; but he regarded them as cases, not of congestion but of anæmia.

Dr. JEWELL replied that the distinction was to him quite clear, though difficult to define, yet he did not despair of doing this.

Owing to the late hour, upon motion of Dr. Spitzka, the discussion of Dr. Jewell's paper was postponed until the beginning of the following session.

Upon motion of Dr. Spitzka, the Recording Secretary was authorized to cast the vote of the Association for the officers which had been nominated for the ensuing year, which was in the affirmative.

The President declared the Association adjourned.

Second day, afternoon session.

The meeting was called to order by President Bartholow, at 2.30 P. M.

Present : Drs. Amidon, Bartholow, Beard, Birdsall, Gray, Gradle, Hammond, W. A., Hammond, G. M., Jewell, Kinnicutt, Mills, Morton, Ott, Rockwell, Spitzka, Seguin.

The President announced that the members of the Association were invited to be present at a reception at Dr. Wm. A. Hammond's house, at nine o'clock in the evening.

The Secretary read the minutes of the previous session, which were approved.

The Council reported that they had examined the paper of another candidate, that of Dr. Wharton Sinkler, of Philadelphia, and recommended that he be presented to the Association for election.

Upon the motion of Dr. Spitzka, the Secretary was authorized to cast the vote of the Association, which was in the affirmative.

The Secretary read letters from the following absent members : Drs. Robert T. Edes, J. Van Bibber, and J. J. Mason, the latter inviting the members of the Association to a meeting in Newport.

Discussion upon Dr. Jewell's Paper. (Continued.)

Dr. SPITZKA remarked that he had made some experimental researches upon strychnia, and had observed some very remarkable phenomena. He would not dare to give strychnia in myelitis. He had artificially produced myelitis in dogs by means of the application of ice-cold water to their hind quarters. Experimenting in this way with two dogs, to one he gave a poisonous dose of

strychnia, and then killed both of them. In the case of the dog to whom strychnia had not been given, there was found striking pathological softening; but in the other dog, no change whatever was found. In these cases both dogs had been subjected to the same influences which are known to produce myelitis, and if any thing could be drawn from the experiments it would be to the effect that strychnia was of use in myelitis. Experiments on frogs show that strychnia had a local stimulating effect. Dr. Spitzka thought it was a molecular change which was produced, and accordingly, experimentally, it was advisable to use strychnia in the first stages of myelitis; however, he would not like to do it. Opinions formulated in regard to myelitis seemed to be vague. As far as he could judge there were cases of myelitis and hysteria that went together; that is, he meant that there were cases of myelitis with irritation where strychnia would be counter-indicated. There was a class of hysterical patients in which the majority bore strychnia well, but he had seen strychnia do harm and produce characteristic symptoms when it was given in small doses within the normal limits.

Dr. HAMMOND wished to ask Dr. Spitzka whether, in the case of the dog to whom he had given a poisonous dose of strychnia, he did not find the blood-vessels in the substance of the cord ruptured.

Dr. SPITZKA replied that whenever he gave strychnia in sufficient doses to produce death immediately, or very soon, that he had found hemorrhages.

Dr. HAMMOND enquired if such hemorrhages were not due to congestion of the cord.

Dr. SPITZKA replied that he had always considered them as due to respiratory interferences, and he could produce death by strychnia without congestion of the cord.

Dr. HAMMOND wished to enquire whether, in the case of congestion of the cord, the spinal cord was not rendered more susceptible to the influence of strychnia than when it was not congested. He wished to have this point discussed. He did not doubt Dr. Jewell's facts or results, but he did question his diagnosis, and did not believe that they were cases of congestion of the cord; but, on the contrary, thought they were cases of anæmia of the cord, otherwise strychnia would have produced its physiological symptoms. He referred to the questions he had asked the previous evening, and particularly to the distinguishing points between anæmia and congestion.

Dr. JEWELL remarked that he had not concluded his paper on account of its length and his fear of worrying the members of the Association by reading reports of cases. First of all, his object was to call attention to the early and free use of strychnia in what he had regarded as subacute myelitis and related diseases of the spinal cord. He had referred only to the practical aspects of the cases, knowing full well that their diagnosis would be questioned, and he was pleased that it had been done. He was aware that much confusion of opinion existed, especially in regard to the diagnosis, between spinal anæmia and spinal congestion, and he had pursued his studies in full view of that fact ; but for one he could not admit that our knowledge on this subject was in such a confused and chaotic state as some seemed to think. He thought that in ninety-nine cases out of a hundred we could differentiate more or less clearly between spinal anæmia and spinal congestion. He alluded to acute and passive congestion, and by the latter term he did not mean that which was of purely mechanical origin. That acute and passive congestion of the central nervous system existed no one doubted, and all would probably agree that we could diagnosticate congestion of the nerve centres. Difficult as it might seem, a diagnosis could be made between passive congestion and anæmia of the cord. By passive congestion he meant that which was of purely vaso-motor origin, and pertained not to the veins, but to the arteries and capillaries, the former of which were almost purely muscular in their middle walls, and were supplied with local vaso-motor mechanisms. The congestions which arose in consequence of loss of tonus in the muscular wall of the blood-vessels were those he had in his mind, whether due to loss of power in the muscular tissue itself or to loss of power in the vaso-motor system. In either case the vessels dilated under the influence of the expansive pressure of the passing blood. When this happened he considered that we had passive congestion, and it was this state which he assumed existed in the blood-vessels of the spinal cord or brain, especially when we consider the fact that they are generally surrounded by spaces, truly called perivascular, so that a better chance for expanding was offered than was found in other parts. As to the difference between spinal anæmia and spinal congestion, he would make the following points : Cases of spinal anæmia were made better by increasing the atmospheric pressure ; or, in other words, by sending patients thus affected from high altitudes to the seaside. Cases of spinal anæmia were better when the barometer stood high, and especially so if placed in a

chamber where atmospheric pressure was increased ; and they were made worse when the barometer stood low, and by removing them to mountainous regions, or by exhausting the air from a chamber in which they were placed. Passive congestions were made worse, as is easily understood, when the barometer rises. If such cases were sent from the region of Chicago to the sea level they did not improve ; but if sent to higher regions, such as Colorado, they got better.

The mechanism of the case is exceedingly simple. The increased pressure upon the body forces the blood from the surface into the air-tight cerebro-spinal cavity, so that the vessels within weakened vascular areas give way according to the degree of weakness of the muscular coat or interference with the local vaso-motor apparatuses. Such cases he never sent to cold regions, but always to warm regions. That was the case with spinal congestion, not so with anæmias. These latter cases were made better by exposure to cold. The contraction of the cutaneous blood-vessels thereby brought about, displaced more or less of the blood naturally circulating in the exterior, and caused a corresponding increase in the amount of blood that circulated in the interior of the body ; this could be demonstrated experimentally. He had practically demonstrated this matter to himself, and was thoroughly convinced that cases of spinal anæmia were invariably better by moderate exposure to cold, while cases of spinal congestion were in various degrees made worse. And conversely, spinal anæmia was made worse by heat to the surface ; but spinal congestion was made better on account of the blood flowing more freely in the surface. The dorsal decubitus benefited spinal anæmia, but not passive congestion. Then, again, in passive congestion there was marked diminution of all the reflexes effected through the congested zones of the cord. By the term vaso-motor anæmia he meant that rather rare condition which resulted from a contraction of the blood-vessels in consequence of a change in the action of their vaso-motor nerves. This could usually be traced to some source of irritation, as in the pelvic, gastric, genito-urinary, or other zones.

Again, electro-muscular excitability is diminished in general passive spinal congestion, but not so or to the same degree as in vaso-motor anæmias of the same regions. There were various other points, a consideration of which would aid in settling positively whether we had to deal with anæmia or cases of passive congestion.

As regards frequency, he remarked that spastic contraction of the blood-vessels of the spinal cord, continuing for a great length of time, was a rather rare phenomenon; but as to congestion it was not an uncommon thing for it to occur and to continue for a long time. He spoke of cases of so-called spinal irritation supposed to be due to spinal anæmia. These he had been led by observation to divide into two sections; one belonged with those cases of pachymeningitis and other diseases of the envelopes of the cord in which there was not simply hyperalgia, but actual soreness and not simply tenderness and pain. These cases were frequently considered as instances of spinal irritation. Besides these there was another class in which certain zones of the cord became greatly exhausted, there being in these zones a loss of balance between the processes of waste and repair, until the structure of the cord became worn and irritable—hyperalgie,—so that slight sensory impressions entering the affected regions were interpreted as being severe. The pain was not due to inflammation of the cord, or congestion, or anæmia of the cord; it was a matter of nutrition. For example, a diseased spinal pelvic zone might arise from irritative disease of the pelvic organs; in the gastric zone the spinal disorder might be due to gastric catarrh; and so on, where any part of the cord had been greatly over-used in nutrition or irritated by peripheral disease, it became unbalanced in nutrition, and one of the early results was pain. This was what happened in cases of true spinal irritation, but they were not necessarily due, in his judgment, either to anæmia or congestion of the cord.

Dr. HAMMOND remarked that it was rather singular, in view of what had been said, that Dr. Jewell, living in Chicago, should see only cases of spinal congestion, and he, living in New York, on the sea level, should see only cases of spinal anæmia. He thought that Dr. Jewell's remarks were based upon transcendental pathology, and that his argument was begging the question altogether. Because Dr. Jewell's patients got better under the use of strychnia, therefore, it was concluded by him that they were suffering from congestion. Dr. Hammond gave strychnia to patients who improved under its use, and he considered them examples of anæmia. He was glad to hear Dr. Jewell admit that cases of spinal anæmia got better in the recumbent posture. He thought that Dr. Jewell's remarks proved just exactly the opposite of what he thought they did.

Dr. SEGUIN remarked that the subject was one of great impor-

tance, and his excuse for prolonging the discussion was that he had very firm convictions in regard to anæmia and hyperæmia of the spinal cord, and the possibility of making a diagnosis between them, and the therapeutics of this class of cases. It was, perhaps, because he had tried to study the disease of the nervous system from an anatomical standpoint, rather than through therapeutical and physiological views, that he had felt opposed to the accepted ideas in regard to anæmia and hyperæmia. With regard to the brain, he thought there were rare cases of hyperæmia and anæmia, but as for the cord, he considered the conditions of hyperæmia and anæmia as purely hypothetical, for he knew of no tangible evidence to support such views. He knew of none but clinical and therapeutical phenomena to prove the existence of such conditions, and he regarded Dr. Jewell's argument as nothing more or less than begging the question. There was no solid foundation for the doctrines of spinal anæmia and hyperæmia as there was for locomotor ataxia, myelitis, and various other forms of spinal disease. He thought the whole modern doctrine of inflammation was opposed to hyperæmia being a cause of inflammatory action. Probably a more important factor was the condition of morbid activity of cells. With respect to the spinal cord, the researches in pathological anatomy had not shown any basis for hyperæmia being considered the first step in myelitis. Cases could be divided into three *post-mortem* categories: First, those in which the ganglion cells and fibres were primarily affected. Second, those in which the connective tissue was involved primarily; and, third, degenerative myelitis. But in any of these he would defy any pathological anatomist to point to hyperæmia as an important factor. In his specimens there were no evidences of hyperæmia; all the changes were tissue changes from first to last, and there was no tangible evidence of increased vascularity. Within two or three years several specimens had been shown to the Association which would bear him out in this statement. The specimens obtained from Dr. Webber's case, as well as his own, did not justify one in believing that there was hyperæmia of the spinal cord previous to the inflammatory action. With reference to the practical observations of Dr. Jewell's, he agreed. He would suggest that the term subacute myelitis be altered to subacute *diffused* myelitis. In the cases of myelitis transversa, it made no difference whether acute or subacute. Strychnia, produced tonic spasms in the paralyzed limbs, whereas in the diffused forms of myelitis he had experimented with this drug

apparently with the best results. During the past spring he had a case of extensive diffused myelitis following anal diphtheria, in which he administered strychnia early with the best effects. He was prepared, if a case of acute diffused myelitis came under his treatment, to give strychnia a little more heroically than before having heard Dr. Jewell's remarks. He agreed with the author of the paper as to the beneficial effects of massage and rest.

Dr. GRAY remarked that he understood Dr. Jewell to advocate the use of strychnia in cases advanced beyond the commencing stage, or, in other words, that its use was not to be limited to the early stage.

Dr. JEWELL said that in his own practice he had not only used strychnia in the early stages, but most of all in the later stages, after the acute symptoms had passed away.

Dr. GRAY remarked that he had tried strychnia faithfully in two cases of what might be called transverse myelitis, and had obtained the physiological effect of the drug without deriving any benefit whatever from its use, both cases having lasted five or six months. He did not know but that the point to which Dr. Seguin had called attention would explain this.

Dr. SPITZKA said that he had always held the same view in regard to this question hypothetically, but had not called attention to it, for he thought that few things could be better demonstrated than that the treatment of transverse myelitis must be different from that of diffused myelitis. He had felt somewhat embarrassed by the theoretical view of Dr. Jewell that hyperæmia was an initial factor in inflammation.

Dr. JEWELL remarked that he supposed that he owed it to himself, that he had been misunderstood. He had not been talking of how congestions arose, but of congestions. Dr. Hammond had misconceived his remarks. He agreed with Dr. Seguin in regard to hyperæmia not preceding inflammatory action. He had not the slightest doubt but that a process of irritative molecular change antedated the active congestion of inflammation. He believed strychnia acted, not upon the blood-vessels, but upon the nerve tissue. As to the far-reaching scepticism of his friend, Dr. Seguin, in regard to anæmia and hyperæmia, it appeared to him his friend apparently believed in nothing in medicine except what he could see, smell, feel, or physically demonstrate. He partially shared this feeling, but, though he admired caution, he was not in sympathy with such a general nihilistic movement against theorizing in medicine, for he thought that when we saw certain evi-

dences which were not to be appreciated wholly by the senses, we could at times step out, not into the dark, but, guided by the hand of rational inference, into the light of new knowledge.

The next paper was by Dr. F. J. Morton, of New York, upon "A new current of induced electricity."

Remarks.

Dr. BIRDSALL thought that it was unfortunate that Dr. Morton had chosen the terms which he had to designate his current. He referred in one case to it as a faradic current, and again as a static induced current. Since Faraday's time an induced current had always been considered as flowing in the opposite direction to the inducing current, and occurred only when the current was closed; it was the reverse when it was opened. He did not understand that this was the case with Dr. Morton's current. Though in truth it was in one sense an induced current, yet as Faraday had attached the term induction to a different phenomenon, he thought it was improper to use it in any other sense, as it would lead to a confusion of terms.

Dr GRAY remarked that the point in the paper which had interested him more particularly, was that in regard to its practical value. He would like to ask whether the pain produced by the new current was much less than that of the ordinary faradic current. If so, it would be of great value in the treatment of children.

Dr. MORTON thought that Dr. Gray had suggested the pith of the whole matter, viz., whether the new current could be made of any important use. One difficulty was to be found in the machine. Even if machines were made that could be operated at all times, their construction was of such a nature that they were not portable. However, the current was of so much use, he believed that many would have these machines placed in their offices. As to the painfulness of the current, this was a matter merely of comparison. The softness and agreeableness of any induction current would depend upon the extreme fineness of the wires. This electricity was of so high a tension, that it was very soft, unless interrupted. If the sponges were well wet with it, a contraction could be obtained, such as would be produced with a strong induced current. He had found in his office practice that children bore the current well.

Dr ROCKWELL confessed that he was a little astonished that the question could be brought up in regard to the comparative merits of static electricity and faradic electricity. In regard to pain, he had learned from practice that the faradic current was not at all painful. He could produce contraction of every muscle of the body with a faradic current, without the slightest sense of pain. It was exceedingly pleasant ; not disagreeable. In regard to the therapeutic value of the two currents, he considered it was impossible for one to testify unless he had used both currents thoroughly. The static electrical current was one which could not be utilized extensively.

Dr. MORTON remarked, that he would add a word, simply in defense of the name. The current was as much of an induction current as any faradic induction current, being simply an electric influence set up by a conductor through space, by the presence of an active source, either of mechanisms, galvanic current, or other sources. It did not make any difference what kind of electricity was used ; it mattered not what was its source. For the purpose of illustration, he stated that the Leyden jar corresponded to the galvanic current, and that the electricity supplying the inside of the jar was, as a general thing, positive, and by means of induction we had on the outside, negative electricity. The induced current was only a transitory current set up through a dielectric. The sparks corresponded to the making and breaking of an ordinary induction coil, and whether the spark was long or short, as in the common coil apparatus, was due to whether the hammer struck fast or slow. He was of the opinion that the new current was very perfectly induced. As to Dr. Rockwell's criticism, the painfulness of a current depended upon its strength. We could use it so as to give absolutely no pain ; but for certain electrical reactions he believed it was found necessary to use a faradic current which gave some pain, which, however, depended upon the operator, and strength of current used, the whole matter being one of comparison.

Dr. BIRDSALL enquired if the direction of the current was the same or opposite to the inducing current.

Dr. MORTON replied, that he had not been able to tell whether the induced current corresponded to the make or break, or which occurred first, or whether it corresponded, or was opposite, to the direction of the inducing current.

Dr. BIRDSALL remarked that a truly induced current always flowed in the opposite direction from the current which induced it.

Dr. SEGUIN inquired what was the relation of the current as regards the time of its occurrence? A true faradic current occurred at the moment of the break of the inducing current.

Dr. MORTON remarked that as soon as the circuit took place there was an equalization of the electricity in the two jars and the outer tin-foil.

Dr. SEGUIN said that the faradic current occurred at the moment when the other ceased. He was of the opinion that we must distinguish two kinds of induction, viz. : induction in general, and the induction of Faraday, which occurred at the moment of cessation, or at the instant of the appearance of the current in the inducing circuit.

Dr. MORTON remarked that there was constantly an alteration in the direction of the currents, but that in batteries now in use the current took place only in one direction, being so constructed for convenience. In the new current we had alternating currents of even strength, and in this respect it differed from the ordinary faradic current, in being more perfect.

Dr. GRADLE, of Chicago, remarked that the discharge was independent of the charging of the jar, for when these were once charged, they would discharge as soon as the connection was made. There was a constant accumulation of electricity upon the inner coat, and a separation upon the outer coat, the constancy only being interrupted by discharges when the tension became high enough to overcome the resistance inter-opposed. He was of the opinion that the new current was not induced.

Dr. MORTON said that he would agree with Dr. Gradle, if in the case of the production of his current there was only a discharge corresponding to that of an ordinary Leyden jar ; but the discharge was not the same as that of an ordinary Leyden jar ; it was a true current, for it had different potentialities.

There being no further discussion, Dr. G. M. Hammond, of New York, read a paper, entitled, "The hypothetical auditory tract, in the light of recent anatomical observations."

At a meeting of the New York Neurological Society, on February 1st, of this year, the author had read a paper describing and giving the measurements of certain gigantic nerve cells discovered by him, and showed by comparison that these cells were larger as far as the carnivora were concerned, than any of the giant cells described by Betz. From the brain of the same cat in which he discovered the giant cells before mentioned, he mounted some

one hundred and fifty sections cut transversely to the cerebral axis, and including that portion of the brain between the lower olivary altitude and the optic lobes of the corpora quadrigemina. These sections enabled him to make a thorough study and examination of the cells contained in the optic lobes, nucleus tegmenti, and auditory nucleus. These cells were not a new discovery. They were known to Meynert, and their dimensions in the human brain had been given by him ; but the author of the paper was not aware of any one having given any comparative measurements of the cells. The author gave the measurements and descriptions of the cells, of the optic lobes, nucleus tegmenti, and auditory nucleus, from sections taken from the same brain, and compared them with the cells of the cortical group discovered by him. A microscopical demonstration of these cells was afforded the members of the Association. These three groups of cells followed the same law of progress as the sensory cells, that is, where there was an increase in the number of cells in the lower groups there was also an increase in the higher ones, and this increase in the higher groups was greater in proportion to that in the lower. For example, just as there was a progress in the development of cells of the anterior spinal cornu in the frog as contrasted with the proteans, so there was a still greater increase in the reticular field in the lower mammalia as contrasted with the reptiles and amphibians, and a still more rapid stride in the higher mammalia over the lower mammalia, in whom these cortical cell-groups were really absent. This anatomical fact was in parallelism with the physiological observation that the simple reflex acts were the common property of all animals, low and high ; that progress in functions was first manifested in the development of coördinated reflexes, which, in their turn, were merely stepping-stones for the highest nervous combinations of psychical life.

Remarks.

Dr. SEGUIN wished to call attention to the pathological findings in an interesting case of aphasia published in the *Archives of Medicine*, April, 1881. This case was that of the late Dr. Allin, whose aphasia was characterized chiefly by word-deafness, and who had no appreciable paralysis. The autopsy revealed a patch of softening destroying the inferior parietal lobule, a region which in many respects was identical with the cortical areas which Ferrier's and Munk's experiments had shown to be intimately con-

nected with the functions of hearing and sight in monkeys and dogs. He believed this case might serve as a clinical and pathological support of Dr. Hammond's anatomical deductions.

Dr. HAMMOND remarked that in his specimens he found a great number of cells upon the left side, while upon the right there were a very few.

Dr. SPITZKA said that he had, on the previous day, an interesting case of congenital atrophy of the cerebellum in a patient markedly ataxic, and yet his musical sense was unusually developed. And though the patient was practically an imbecile, he could repeat, after once hearing, classical operas.

The next paper was by Dr. George M. Beard, of New York, on "The medical use of statical electricity, or franklinism."

The history of franklinism in medicine had been one of tremendous expectation and tremendous disappointment. He said Berge had constructed a statical machine that would go at all seasons of the year. This was of great advantage, and would enable him to test whether there were or were not cases in which this form of electricity would be superior to either faradism or galvanism. The question was not whether this form of electricity produced a sedative or tonic effect. That had been determined a hundred and fifty years ago. The question was whether the sedative and tonic effects differed from those of faradism or galvanism. This question had not been answered. The publication of cases treated by statical electricity proved nothing except when compared with the effects produced by other forms of electricity. We could not say that franklinism was superior to faradism or galvanism unless we had used the others, and thereby derived a standard of comparison. We were now in a position to settle the question, though it would not be found an easy thing to do, and would take a long time. He stated that he was using franklinism every day with his patients. The current of Dr. Morton was, he thought, incorrectly named. It was induced, but all the phenomena of static electricity were phenomena of induction. The current from the outside of the jars was really secondary static electricity, and he thought this would be the proper name by which to designate it. It produced muscular contractions; it was milder and easier of application.

Remarks.

Dr. ROCKWELL, some four or five years ago, had experimented with statical electricity, and had drawn certain conclusions from

his experience. At the beginning of its recent revival he had procured a new machine, but as yet he had no reason to change the opinion formed some years ago. He did not care to be destructive in criticism, therefore he would say that the absolute value of statical electricity was very great; but in comparing it with other forms, its range of usefulness was inferior, and far inferior to the two forms of dynamic electricity combined. In electricity, as in medicine, benefit was often derived from a change. We know that when a certain tonic had been given for a considerable length of time, the patient improved if it was changed for some other tonic, though it was known to be inferior to the first. Therefore, for this reason, he would recommend its use. He quite agreed with Dr. Beard in the choice of galvanism first, faradism next, franklinism last, but, preferably, all three.

Dr. AMIDON said that what little experience he had, while with Dr. Charcot, in Paris, led him to agree with Dr. Beard. Most commonly the good effects of statical electricity were due to mental impressions. The best results which had been obtained in Charcot's practice were in cases of hystero-epilepsy. Drs. Charcot and Vigouroux claimed that only in this way was it superior to galvanism and faradism. They used it in a variety of cases and on a large scale; they would huddle together upon a single isolated stool cases of hystero-epilepsy, locomotor ataxia, paralysis agitans, a case of anæsthesia, and a case of headache, thus forming a series of pathological conditions which had nothing in common, and apply the same current to all of them. He considered this wholesale way of dealing with patients not advisable, and that it was adopted only as a means of saving time. He had noticed that when Dr. Vigouroux had a case of infantile paralysis he took it to the galvanic machine and applied the current with great care. Dr. Onimus, one of the best electricians in Europe, never used static electricity. He had watched the application of static electricity, had taken histories of cases, and with the exception of temporary relief in hysterical patients, he had never seen any benefit derived from its use.

Dr. BIRDSALL remarked that he observed that Dr. Beard held partially to the view that while the general term induction was applicable to Dr. Morton's current, the term was not well taken. He thought we should be exact in regard to the use of our terms in these matters. The name given by Faraday should be maintained as describing a particular condition. He would state again that he considered, as Faraday had considered, a current in a conduc-

tor to be an induced current, when it was produced by another current or magnet at the moment when the circuit was made and broken, and that the current ordinarily passed in the opposite direction from the inducing current. In regard to its uses he could confirm to a slight extent Dr. Amidon's statement.

Dr. MORTON thought that Dr. Amidon took rather a humorous view of Dr. Charcot's use of statical electricity. Charcot and his associate Dr. Vigouroux had to administer to the wants of a large number of patients, and he saw no inconsistency in giving electricity in the manner described. Professor Charcot had recently written a long article, in which he had analyzed the historical position of static electricity, together with the different machines and appliances which had been used, and then proceeded to state the class of cases in which he thought its use was of value. Hysteria was one of the diseases in which he thought it of great value. He stated that static electricity was of the greatest value in a large number of diseases.

Dr. ROCKWELL wished to ask Dr. Morton if he had ever put to a thorough test the tonic effects of general faradization in all its power.

Dr. MORTON replied that he had never used general faradization.

Dr. BEARD remarked that the question was not whether the static electricity helped to cure disease, nor was it what Charcot or Vigouroux thought of it. That question had been settled even in this country before Charcot or Vigouroux were born. The question was whether there was any comparative superiority of the two forms of electricity, and he thought this was the only thing to be considered. He thought there were a great many gentlemen in this country and in Europe who could make the comparison between the different varieties of electricity, and who knew what electricity could do, and that they would make this comparison.

There being no further discussion, Dr. Beard read a second paper giving directions "How to use the bromides."

He regarded the bromides as among the great and few remedies which we had, and that they ranked with opium, quinine, and electricity. As far as we knew, their good effects depended upon their being administered in the treatment of functional nervous diseases. What he had to say had especial reference to other diseases than epilepsy. Their use in epilepsy had been much writ-

ten upon, but it was not so well known that they were of value in the treatment of various functional nervous disturbances or diseases, though in these conditions they had proved as efficacious as in epilepsy, and far more so. In giving the bromides for the above-named conditions, the object aimed at was first to produce the effect of bromization to a greater or less degree. When the bromides were given in such small doses that they did not produce bromization, they did not accomplish much good for the patient. Bromization was an abnormal state, a disease, but in therapeutics we cured diseases by producing disease. Second, to rapidly induce bromization it was usually of advantage, though not absolutely necessary, to give immense doses, from 30 to 100 grains, more or less. Idiosyncrasies were sometimes met with, where patients were susceptible to small doses of the bromides. They should be watched for. In some cases, as in attacks of hysteria or sea-sickness, a single large dose, say 100 or 120 grains, or more, given in one or two tumblers full of water, would be sufficient, without any more, to accomplish the purpose for which they were given. To sea-sickness bromization was what vaccination was to small-pox; it absolutely prevented it in nearly every case. There was no nerve disease known to science so absolutely under medical control as sea-sickness. Third, the bromides were to be given in these immense doses, for a short time only, save, of course, in epilepsy and epileptoidal conditions—a few days, sometimes two, or three, or four, or more days. The secret of success in the use of the bromides, as with every thing else, is to know when to stop.

It was because of the want of this knowledge that we heard so much about the evil effects of the bromides. He cited cases where bromization had been produced within half an hour, and stated that it was possible to kill a person with the bromides as surely as with a pistol. In some cases bromization sprung upon one suddenly after a long delay; it did not usually creep upon the patient slowly. Fourth, the bromides, if used long or frequently on any patient, should be used alternately with tonics; this was very important and not generally known in connection with other diseases than epilepsy. Fifth, it was of advantage to use a number of the bromides in combination. The bromides which he usually used in combination were the bromide of potassium, calcium, sodium, ammonium, and lithium. He also used other bromides such as the bromide of camphor, zinc, and iron. Sixth, some nervous patients, who were not epileptic or epileptoidal, needed to

use the bromides for a time, just as though they had epilepsy, subject to the directions above given. There was such a thing as the habit of taking the bromides.

Remarks.

Dr. HAMMOND enquired if Dr. Beard attributed the effects to the bases, or to the bromine. He said that he had been somewhat struck with the idea that the same effect, in epilepsy, could be obtained from thirty grains of a salt, three times a day. He was inclined to think that the bromide condition was a modified condition of scurvy; he had been giving the pure bromine in drop doses, and had obtained the same effect upon the epileptic phenomena, but without giving rise to scurvy. He simply mentioned it as a point needing investigation. So far as the influences which particular bromides had, he did not see any particular difference between them, or any advantage in combining them. He almost invariably administered bromide of sodium because it was more pleasant to the taste.

Dr. GRAY remarked that there was one source of fallacy in getting at the effects of the bromides, which was almost universally overlooked, viz., that these effects were generally estimated from their use in cases of epilepsy. After having given one of the bromides until its good effects had ceased to be manifest, as, for instance, with the bromide of potassium, if it were stopped, and the bromide of sodium were given instead, good effects would follow its administration, and after this failed to act, if the patient was put upon the bromide of ammonium he would improve. Not only so, if their administration were stopped altogether, the patient would get better, or if he was put upon almost any other remedy, or if they became the subjects of an injury, and were confined to the bed, they did not have epileptic seizures for a long time at least. A tap on the head would also, sometimes, stop epileptic convulsions for a few days. Hence, one in an enthusiastic frame of mind might attribute undue therapeutic value to some particular bromide or plan of treatment, when, in reality, epileptics improved upon almost every new remedy which was administered to them.

Dr. SEGUIN rose to protest against the comparison of bromization to scurvy. He had a very clear picture in his mind of the latter condition. He had seen a shipload of scorbutic sailors with multiple hemorrhages, some of them as large as a hand,

without any nervous symptoms. The tendency to hemorrhage was characteristic of scorbutus, not so in the case of bromides. Their administration was followed by loss of power. In regard to Dr. Hammond's question, he thought he had answered that in a series of experiments which he performed while in charge of the hospital for epileptics on Blackwell's Island, in 1874. He tested the comparative value of the chloride of potassium, and the bromide of potassium, with the following results: Three male patients one month under KBr, 22 attacks; under KCl, 115 attacks. Eighteen female patients, one month under KBr, 205 attacks; one month under KCl, 410 attacks.

Again, thirteen female epileptics under KBr, average monthly number of attacks in a period of three months, 70 attacks; the same, one month under KCl, 348 attacks.

These results were published in the *New York Medical Journal*, April, 1878. He had come to the conclusion that the efficient agent was bromine, and not potassium. Upon most points as regards administration he agreed with Dr. Beard.

Dr. HAMMOND wished to give the Association the formula which he used, which was one drachm of bromine to eight ounces of water. Of this he gave a teaspoonful, properly diluted, three times a day.

Dr. JEWELL remarked that he had had a somewhat similar experience in regard to the use of bromine, and merely rose to declare it. He thought some patients would bear the bromine, when they could not take it in connection with the alkaline base. He noticed this particularly in one patient who had been taking the bromides constantly, he believed, under the direction of one physician or another for thirteen years, and who was obliged to abandon them entirely on account of their bringing out immense sores, especially upon the legs. This same patient had been using bromine alone for a considerable while without any return of the disorder spoken of.

Dr. SEGUIN considered Dr. Jewell's observation a very valuable one. A recent patient of his had an eruption produced upon her legs in a few days by moderate doses of the bromide of potassium. The same condition was caused by mixture of chloral and bromide. He had placed this patient now upon bromide of camphor, which controlled the epileptic fits without giving rise to cutaneous lesions. He wished to inquire if this eruption had been noticed principally upon the lower members.

Dr. JEWELL remarked that the eruption in his case was found

upon the legs from the knee down, and also on the face, in the distribution of the trigeminus, in which location it was very bad.

There being no further discussion, upon motion of Dr. Hammond, the Association adjourned.

Third day, afternoon session.

The Association was called to order by President Bartholow, at 2.30 P. M.

Present.—Drs. Amidon, Bartholow, Beard, Birdsall, Gradle, Hammond W. A., Hammond G. M., Jewell, McBride, Miles, Morton, Rockwell, Seguin, and Spitzka.

The Secretary read the minutes of the previous session, which were approved.

The Council recommended the acceptance of the resignation of Dr. Cross.

Upon motion of Dr. Spitzka, the resignation of Dr. T. M. B. Cross was accepted by the Association.

The Secretary announced the receipt of excuses for absence from Drs. Eads and Putnam, of Boston.

The amendment to the constitution offered by Dr. Gray, upon motion of Dr. Hammond, was deferred for action, in consequence of the absence of its author.

The first paper was by Dr. Wm. A. Hammond, of New York, entitled, "Nerve-stretching in locomotor ataxia."

His paper consisted of an enumeration of the published accounts of cases in which the operation had been performed, together with the history of the cases in which he had operated. His practice had been to expose the sciatic nerve on the posterior part of the thigh at about the junction of the upper with the middle third, and, introducing his little finger, pull alternately up and down, until the nerve had been stretched about an inch or less, when it was returned to its bed and the wound dressed antiseptically. He was led, by the favorable results attained in his three cases, to the opinion, that the operation might prove of decided value.

Remarks.

Dr. JEWELL reported that he had recently received information by letter from a well-known physician of the successful

performance of the operation in two cases in which it afforded marked relief. The history of these cases was unpublished, so far as he knew, and they should be added to those in which the operation had been performed with benefit.

Dr. SPITZKA, without wishing to adopt the opinion of the author of the paper on European operations, considered the amelioration of pain following the operation no positive evidence that it was in consequence of the operation. The case of a physician in the U. S. army had recently come to his notice, in whom such remarkable symptoms were caused by the taking of morphine that the diagnosis from posterior sclerosis of the cord was exceedingly obscure. He had, however, unquestionable disease of the spinal cord. The suspension of the morphine caused a sudden cessation of the pain. The same claim had been made for static electricity in ataxia that was now made for this operation, and until the proof was more conclusive than at present he thought we had better reserve our opinion as to the curative or beneficial effects of the operation.

Dr. BIRDSALL referred to Dr. Westphal's case, and said that in that instance no lesion of the spinal cord was found, and that during life a good deal of doubt was expressed as to whether it was a case of locomotor ataxia. There was no degeneration of the posterior columns, and the history given by Dr. Langenbeck was exceedingly meagre. He stated that the disease developed within a few months, and that at the time of the operation the patient had ataxic symptoms in the upper and lower extremities. He did not describe the nature of the disease. As to the existence of pains and the absence of the tendon reflex before there was any change in the spinal cord, he considered it a doubtful point, and one that could hardly be credited. He might, perhaps, refer to a case mentioned by Dr. Seguin in a published paper. In this case there was pain and absence of tendon reflexes for 30 years, without any ataxic symptoms being manifest. Dr. Birdsall did not mean to say that doubt should be thrown upon all these cases, but probably a great many cases would be met with that would not be carefully examined, and a diagnosis not carefully made. In a certain number of cases the symptoms to which Westphal referred were acute and disappeared rapidly. In regard to the effect of nerve-stretching, the experiments of Brown-Séquard in stretching the nerves of animals were probably familiar to the members of the Association. The general view which was gaining ground, that the effect of stretching a nerve did influence the

central nervous system, at least temporarily, was one that was deserving of a good deal of consideration; but he judged that the effects were to a great extent temporary, and perhaps would be followed by bad ones, as happened in Westphal's case.

Dr. SEGUIN remarked that it had always seemed to him that sclerosis of the posterior columns was characterized by periods of amelioration, and he agreed with Dr. Spitzka that the subsidence of pains after the operation was not conclusive that it was due to the operation. He had frequently known the pains of sclerosis to be apparently arrested by treatment, and sometimes without any treatment whatever; and from what we know of the disturbing effects of operations upon the central nervous system, it might be that the operation, as an operation, might explain the subsidence of the symptoms. It was known that the operation of removal of the testicle was at one time a favorable remedy for epilepsy, and it no doubt did suspend the attacks for a time. He had no prejudices against the operation of nerve-stretching and might try it, but before doing so he should hardly be led to expect much permanent benefit. He was of this opinion, perhaps, because he believed that the changes in the posterior columns were grave and incurable. In the case referred to by Dr. Birdsall the patient had had pains for 30 years, and he found marked sclerosis in the external part of the posterior columns. He had examined the cord of another patient, that of a man, who for two years suffered from numbness in his legs and arms. The patient died of acute anæmia. There was no ataxia. Before dying he experienced sharp pains in one heel. After the death of the patient Dr. Seguin learned that he had experienced sharp pain in one thigh during the preceding summer while at a water cure; these were the only pains that the patient had had in a two years' illness.

Post-mortem examination showed typical sclerosis of the external part of the posterior columns. He had had an opportunity some two years ago of examining a sciatic nerve stretched by Dr. Weir for tetanus, and he found very few nerve fibres in a state of degeneration. There was marked perineuritis at the seat of handling, but the inflammation did not seem to proceed very far within the bundle of the nerve, and he was quite surprised at the comparatively healthy condition of the nerve.

Dr. AMIDON called attention to the fact that in Dr. Weir's case, besides stretching, the nerve was taken upon the curved side of a director and rubbed. He enquired if Dr. Hammond considered the effects due to any thing further than several counter-irritations.

Dr. HAMMOND remarked that he most certainly did. He thought those who first saw a case of tetanus, and made a *post-mortem* examination, would not be disposed to think the disease due to a slight cut in the thumb, but where we know that such slight injuries as this might give rise to so grave a disease as tetanus, he did not think it impossible for nerve-stretching to benefit locomotor ataxia.

Dr. ROCKWELL had no doubt that relief of pain in locomotor ataxia might be brought about by various methods of treatment. He thought, however, that in many of the cases coming from German sources functional disease had been mistaken for organic.

Dr. MORTON had no doubt but that various measures would relieve the pains of locomotor ataxia, and from the experience which he had had in one case with static electricity, he would say that ataxia and pains had disappeared. In reporting this case he had been incorrectly understood as stating that he had cured a case of locomotor ataxia, whereas he simply said that the pains and ataxia were relieved by the treatment. He did not consider it an instance of remission, for it would be strange if the remission should occur just at the moment of treatment, especially when the pains had existed for a long time. In reviewing the observations of Dr. Hammond, it seemed, in view of the amelioration and improvement immediately following the operation, that it was worthy of consideration, especially inasmuch as the operation was extremely simple. He performed the same operation in the case of a patient having lateral sclerosis, cutting down upon the sciatic nerve in the sciatic notch. In this locality he found it a little more difficult than it would be at the point recommended by Dr. Hammond on account of the depth of the nerve. He raised the nerve with his finger, and stretched it vigorously. In the stretching he would avoid the use of instruments. He stretched it until he could feel something give way, and then returned the nerve, and sewed up the wound. The patient wrote that he was immensely better; he walked better. He considered the operation extremely simple, and, in view of this fact, he would stretch as many sciatic nerves for locomotor ataxia as he could get patients who would allow him to do it.

Dr. HAMMOND remarked that some recent experiments had showed the sciatic nerve capable of sustaining a weight of seventy pounds. He spoke of the undue stretching which had been practised in some cases, which in one instance was so ex-

treme that the nerve lay as a loop upon the thigh, having been stretched two or three inches.

Dr. G. M. HAMMOND called attention to a case which had been operated upon by Dr. W. A. Hammond, at the college clinic, and in which the symptoms had been aggravated.

Dr. SPITZKA enquired if the girdle sensation was relieved.

Dr. HAMMOND replied, that in one of his cases the girdle sensation was not relieved. In the other cases there had been none of it at any time, but the bladder symptoms were very much improved, and the tendon reflexes slightly restored.

Dr. BIRDSALL remarked that in a case referred to by him the girdling sensation was not improved, though the symptoms in regard to walking were.

Dr. GRADLE, of Chicago, reported a case of "Spasm of the ciliary muscle of central origin." A young healthy lady suffered from an attack of left hemiplegia during a protracted labor. Both the positive and negative symptoms pointed to a small hemorrhage in the vicinity of the right internal capsule. The motion returned soon in the face and lower extremity, but there persisted a paresis of the arm and paralysis of its extensor muscles. Shortly after attack the patient complained of hazy sight, which trouble did not change in the course of six months. Upon examination there was found an apparent myopia of both eyes, amounting to 1.5 dioptrics. Both eyes were healthy. The myopia was measured with identical result with the ophthalmoscope, but changed to emmetropia on instillation of atropine. The spasm, however, returned after the effect of the atropine had passed off. Very dilute solutions of this alkaloid removed the contracture of the ciliary muscle temporarily, without interfering to an annoying extent with the accommodation.

Remarks.

Dr. SEGUIN remarked that Charcot had called attention to monocular amblyopia in cases of hemiplegia, and he thought that there was no evidence to show that these cases had been carefully examined for errors of refraction, and hence we could not say that they were unlike Dr. Gradle's case. Dr. Seguin had a case of apparent monocular amblyopia, with paralysis upon the right side. He sent the patient to the Manhattan Eye and Ear Hospital, for examination by one of the staff, and it was found that the amblyopia upon the paralyzed side was due to hypermetropia, which must have been an original defect.

Dr. SPITZKA remarked that it had been observed, in general paralysis of the insane, in which the lesions were diffused, that the patients within a short period changed the size of written letters. For instance, they would write letters three-fourths of an inch in height on one day, and the following day, and for months afterward, they would write letters of less than a millimeter in height. It might not be impossible that a condition similar to that described by Gradle might obtain in such cases. These patients were not usually examined carefully. He thought the question introduced by Dr. Seguin was not one that should be confounded with the one under discussion. It seemed to have been referred by its discoverer to a different mechanism entirely.

Dr. JEWELL would simply say that the case reported in the paper was one that had interested him very much, and that as Dr. Gradle had been kind enough to ask his opinion in regard to presenting such a paper to the Association, he had given it as his judgment that it was perfectly suitable, and he wished to say before he sat down (for it was one of his purposes in rising at this time), that it seemed to him that not enough care was likely to be taken in the examinations of ocular manifestations by those who cultivated neurological science. He thought they took place many times unnoticed. He thought it was necessary to investigate the states of the special senses. He had observed, for example, many peculiar phenomena in regard to the color-sense of cerebral origin.

The next paper was by Dr. W. R. Birdsall, describing "A new foot dynamometer."

The instrument described consisted of a base-board eighteen inches by six inches, in which were mortised two upright supports for an iron rod, which formed an axis on which the foot-board turned. Three grooves were cut in the base-board at one end, and corresponding grooves in the under surface of the foot-board, on to which slipped the ordinary elliptical spring dynamometer used for testing the grasp. An adjustable long gauge slid on the upper surface of the foot-board for the purpose of giving a definite position for the foot. In order to fasten the foot firmly to the board, and furnish a point for traction, a broad toe-strap was used when the anterior tibial group of muscles was to be tested, and a narrow heel-strap for testing the posterior group. The method of recording the observations made by means of this apparatus was also explained.

Remarks.

Dr. G. M. HAMMOND gave a blackboard illustration of an instrument which he had invented for the same purpose about a year previous, and which would indicate the amount of pressure in pounds, by means of a spiral spring.

Dr. MORTON remarked that he had used Dr. Hammond's instrument and with the utmost satisfaction. He thought an instrument which would indicate the actual expenditure of force preferable to one indicating only relative force.

Dr. SEGUIN thought that for comparative measurements the instrument devised by Dr. Allan McLane Hamilton was excellent. It consisted of a rubber ball to be compressed, which was connected with a graduated tube filled with colored fluid or mercury.

Dr. MILES, of Baltimore, had not been satisfied with Dr. Hamilton's instrument, except for testing sustained power, for which purpose he liked it very much.

Dr. BIRDSALL remarked that at first he thought of using a spiral spring, but, in order to reduce expense and simplify matters, conceived the idea of utilizing the ordinary hand dynamometer, which most neurologists already possessed. He could see no special advantage in indicating absolute force; but if that was thought necessary it could be done with his instrument by calculating what the force represented.

The Secretary read the following papers by title :

First, a candidate's paper, that of Dr. Burt G. Wilder, of Ithaca, N. Y., on "The Brain of a Hydrocephalous Dog (King Charles Spaniel)."

Second, a candidate's paper, that of Dr. Charles K. Mills, of Philadelphia, "Tumor of the Motor Zone of the Brain."

Third, a candidate's paper, that of Dr. Wharton Sinkler, on "Chorea in the Aged."

Fourth, a candidate's paper, that of Dr. S. V. Clevenger, of Chicago, on the "Function of the Nerve Cells."

Fifth, a member's paper, that of Dr. J. J. Putnam, of Boston, "A Preliminary Notice of an Investigation into the Earlier and Obscure Symptoms of Lead Poisoning."

Sixth, the paper of Dr. E. C. Seguin, "A Second Contribution to the Study of Localized Cerebral Lesions."

Dr. F. T. MILES, of Baltimore, next gave a verbal account of a novel case of myelitis, which he said was one of a class, and therefore interesting.

The patient was a man about 32 years of age, living in the West, in a malarious part of the country. He was seized with symptoms of what his physician called congestive fever,—symptoms of chill followed by fever,—and treated with quinine. He was to be brought to Dr. Miles, but had a relapse. He then suffered from weakness and pain in the lower limbs, as his physician called it. He insisted on continuing his occupation until he became so weak that he had one or two falls, and continued to have pains and numbness in his legs. In this condition he was sent to Baltimore, where Dr. Miles saw him in consultation. His condition was one of almost complete paralysis of the lower as well as the upper extremities, and of the face upon both sides. He could close neither eye, and this gave him a marked appearance. It was supposed that deglutition and his lungs were affected, but Dr. Miles discovered that such was not the case, except he could not grasp the food with his lips. Tongue could be protruded. No tendon reflex nor ankle clonus. There was delayed skin sensation of the soles of the feet; strong tickling of feet gave rise to an exaggerated reflex. There was decided hyperæsthesia upon slight pressure with compasses. No contraction from faradic current, except, perhaps, one or two of the muscles of the legs; none in the upper limbs or face. There was the degenerative reaction of the galvanic current. Dr Miles' prognosis was that he would recover, and he did so completely within two months. There was no bladder trouble, nor paresis of the abdominal muscles. He thought the case illustrated a new phase of myelitis. The case had a remarkable resemblance to polio-myelitis in the loss of faradic contractility and alteration of galvanic reaction. We had here an alteration of the sensitive nerves, an affection not confined, as in polio-myelitis, to the anterior horns, but invading the posterior horns, and, perhaps, the lateral columns. One thing that threw light upon the case, although the gentleman did not admit it, was that he was affected with syphilitic disease.

Dr. MILES thought we frequently saw cases where there was more or less interference with sensorial phenomena in polio-myelitis, and he was of the opinion that we could not say it was an affection which left the posterior horns unaffected.

Remarks.

President BARTHOLOW inquired as to the condition of the respiration.

Dr. MILES had not been able to make out the affection of any of the cranial nerves except the seventh ; respiration was good.

Dr. BARTHOLOW inquired as to the treatment of the case.

Dr. MILES replied that it consisted in the administration of iodide of potassium in gradually increasing doses up to twenty or twenty-five grains, three times a day, and the application of galvanism to the spine and muscles.

Dr. AMIDON inquired if there was any suspicion that the febrile attack might not have been due to a septic disease like diphtheria, and followed by paralysis, which simulated myelitis.

Dr. MILES replied that there was nothing in the history of the case upon which he could hang such a suspicion.

Dr. JEWELL remembered to have had a case, as nearly as might be, similar to Dr. Miles'. The patient was a gentleman who came walking with a couple of sticks into his office one day, and whose face was in the same condition as described by Dr. Miles, or in a day or so after became so. There was no suspicion of syphilis. He had been affected in the autumn and spring with intermittent fever. He continued to improve under treatment for four or five weeks, and went home with the idea of attending to some of his duties in the capacity of principal of a high school. Dr. Jewell told him he could safely go, if he would not undertake any work. He felt obliged to work and commenced walking up and down stairs, got a little cold, and had a return of the symptoms that he had when first taken sick. Dr. Jewell visited the patient at his home two or three times. He was improving again when he had another relapse ; the symptoms became of a very aggravated character. It was now three years since the man had been able to raise himself from a chair, having most remarkable contractions of all of the flexor muscles of the members. There was also stiffness of the muscles of the back of the neck and back, together with very great wasting of muscles. The sensory as well as the motor tracts were affected. This case passed from subacute diffuse, right along into what Dr. Jewell called acute myelitis. The man was permanently ruined in health.

Dr. SEGUIN remarked that he had upon record a case of polio-myelitis, in which the only voluntary muscles that could be moved were those of the eyes, and one of the toes, and left fingers. The face was a perfect mask, there being paralysis of the muscles on both sides. There was no difficulty in deglutition. In this case he thought there was diffuse myelitis in addition to the polio-myelitis. The pains which some of these

patients have were of two kinds. Some had neural or neuralgic pains, and since the publication of Prof. Leyden's paper¹ it had been questioned whether some of these cases of so-called polio-myelitis were not cases of disseminated neuritis. He had thought the same of this case. He had seen a case with Prof. Delafield, in which the pains were of a fulgurating character,—not neuralgic. He was led to infer from the involvement of the posterior columns that a good prognosis was not possible. He had always held that there were transitional forms between polio-myelitis and other forms, and he was prepared to see almost any grouping between simple relapsing polio-myelitis and cases like that of Dr. Miles' and Jewell's, and other cases, as in Prof. Delafield's, where there were indications of involvement of the posterior segments of the cord. What was wonderful about these cases was their curability. Dr. Jewell's case was the only one, so far as he knew, that had not been cured. All of his cases had done well.

Dr. HAMMOND said he had recorded a similar case to the one reported, but had never seen a case where the paralysis extended as high. In one reported in his book the paralysis extended as high as the neck, but the facial muscles were unaffected. The singular feature about the case was the suddenness of the development of the symptoms. Having eaten his breakfast and started down stairs he suddenly found himself unable to move. He fell down stairs, and being brought to New York, Dr. Hammond found his motor functions on the second day after the fall entirely abolished, though sensation was intact. He made a good recovery under the use of ergot and faradism. Dr. Hammond did not believe the treatment had much to do with the result, because, as Dr. Seguin had pointed out, these patients seemed to get well under any treatment.

Dr. SEGUIN inquired if he (Dr. Hammond) did not think there was any localized myelitis.

Dr. HAMMOND replied that he thought the lesion was local, and of the anterior horns, or anterior columns, because there was no perversion of the sensibility, and no paralysis of the muscles of the face.

Upon motion the Association was declared adjourned.

¹ Ueber polio-myelitis und neuritis. *Zeitschrift für Klin. Medicin*, 1880.

Third day, evening session.

The Association was called to order at 8.30 P.M. by the President.

Present.—Drs. Amidon, Bartholow, Beard, Birdsall, Gradle, Hammond, W. A., Hammond, G. M., Jewell, Kinnicutt, Miles, Mills, Morton, Seguin, and Spitzka.

The Secretary read the minutes of the afternoon session, which were approved.

The Council reported through Dr. Seguin that their recommendations for Honorary and Associate Membership were as follows :

Honorary Members.

Prof. J. M. Charcot, Paris ; Prof. J. Hughlings Jackson, London ; Prof. W. Erb, Leipsic ; Prof. C. Westphal, Berlin ; and Prof. Theodore Meynert, of Vienna.

These gentlemen were nominated by the following members : Bartholow, Hammond (W. A.), Jewell, Miles, McBride, Seguin, Spitzka.

Associate Members.

Dr. Thomas Stretch Dowse, London; Dr. Moritz Bernhardt, of Berlin ; Dr. W. R. Gowers, of London ; Prof. David Ferrier, of London ; Dr. Camillo Golgi, of Pavia, Italy ; Dr. H. Charlton Bastian, of London ; Dr. J. Russell Reynolds, of London ; Dr. Obersteiner, of Vienna. Nominated by Drs. Hammond and Jewell. These gentlemen were unanimously elected.

Under the head of miscellaneous business, Dr. E. C. Spitzka moved the adoption of the following rule :

That at as early a date as possible, before the annual meeting of the Association, the members shall be informed by the Secretary of the titles of papers which are to be read at the meeting, and arranged in the order received.

Carried.

Dr. L. C. GRAY gave notice that he had submitted the following amendment to Art. IV of the Constitution at the annual meeting of 1880 :

To read that

“They be nominated by the Association at the first day of the annual meeting,” instead of “They shall be nominated by a Committee on Nomination of five members, appointed by the President on the first day of the annual meeting.”

Upon motion the amendment was adopted.

By a vote of the Association, Dr. N. B. Emerson, of Honolulu, and Dr. J. S. Lombard, of London, England, were transferred from Active to Associate Membership.

In view of further removals from the United States of Active Members, Dr. J. S. Jewell, of Chicago, gave notice that he would submit the following amendment to the Constitution at the next annual meeting :

That all Active Members of the Association who shall hereafter remove from within the limits of the United States shall thereby become Associate Members, should they so desire.

Dr. F. T. MILES, of Baltimore, presented a specimen of "Tumor of the pons." The patient, a woman, was brought into the hospital with motor paralysis of one side; on the opposite side the paralysis was not absolute. She was semi-comatose, which condition continuing for a little time, the cornea became opaque, and she died in this condition. He thought the lesion consisted of a thickening of the dura mater pressing upon the fifth and seventh pairs of nerves at about the points of decussation, but the *post-mortem* examination had proved him to be wrong, there being a tumor in the central portion of the pons. The tumor was supposed to be of syphilitic origin.

Remarks.

Dr. SPITZKA enquired if there were no vaso-motor phenomena.

Dr. MILES replied that he did not recollect of observing any. The trophic influence on the cornea was noticed.

Dr. SPITZKA asked if choked disc existed.

Dr. MILES said that, as far as it could be observed, the Gasserian ganglion was intact.

Dr. SPITZKA called attention to the distortion of the specimen by twisting.

Dr. MILES said that the twisting was in it when he found it.

Dr. SPITZKA thought if that was the case it was one of the most remarkable conditions ever known.

Dr. CHAS. K. MILLS, of Philadelphia, next proceeded to read a paper upon the same subject, entitled "Tumor of the pons Varolii, with conjugate deviation of the eyes and rotation of the head."

The case upon which this paper was founded was one that could not fail to be of great interest to the students of close local-

ization. The patient, R. C., æt. 32, single, groom, had a history of intemperance and of syphilis. He had several times fallen from horses, and had been kicked on the head. Four weeks before coming under observation he had an attack of dizziness and fell, but was not unconscious. A few days later his eyes began to trouble him, and he noticed some loss of power in his right arm and leg. On examination, he was found to be anæmic, weak, and apathetic mentally. He had right hemiparesis. Sensation was diminished on the left side of the face and in the right limbs. Hearing, smell, and taste were preserved. The most prominent symptoms, however, were a conjugate deviation of the eyes and rotation of the head to the right. He could not, by the utmost effort, bring the eyes around even to the median line. Dr. E. O. Shakespeare examined the eyes in addition to Dr. Mills. In attempted movements of the eyes to the left the right eye turned slightly, the left scarcely at all. A slight tendency to ptosis was present on the right side. The power of accommodation was not greatly impaired. The media were clear. The pupils were about normal. The ophthalmoscopic examination of the left eye showed a subacute neuritis. In consequence of the extreme deviation of the eyes to the right, the right eye could not be satisfactorily examined by the ophthalmoscope. A scar and a narrow cleft in the skull were found in the squamoso-temporal region. Two slight scars were also found in the scalp of the right parietal region. The patient was placed upon potassium iodide, and tonics, but did not improve. Persistent epistaxis set in, and was not relieved by treatment. He died of general exhaustion. Before death the face and limbs of the left side became parietic, and right-sided paralysis became more-marked. The pupils became contracted, the left being a little smaller than the right. The conjugate deviation and other symptoms remained about the same.

Autopsy.—A slight cleft or fracture, without displacement or depression, was found in the inner table of the skull, corresponding to the scar and fissure in the squamoso-temporal region. The dura mater was here slightly adherent, and a hard, yellowish tumor, no larger than a pea, was present beneath the adhesion, on the inner surface of the dura. It was attached below to the pia mater also, and caused a slight depression near the middle of the first temporal convolution. On exposing the floor of the fourth ventricle, a distinct bulging of its left upper portion was observed. On making a transverse incision through this bulging mass, a small tumor was discovered in the body of the pons, both the

anterior and posterior surfaces of the latter retaining their integrity. The tumor was distinctly limited to the left upper quarter of the pons, coming close to, but not crossing, the median line. On section it was found to be of firm consistence, and of a greenish-gray color. It was examined microscopically by Drs. J. H. C. Simes and H. Formad, who concluded that it was a gumma.

Dr. MILLS concluded that the peculiar ocular symptoms present in this case were due to the tumor of the pons Varolii. Vulpian, Lockhart Clarke, Prevost, Brown-Séquard, Bastian, and others, have devoted much attention to the subject of conjugate deviation of the eyes, and rotation of the head. This lateral deviation occurs from lesions of various parts of the brain—of the cortex, centrum ovale, capsules, ganglia, crura cerebri, and pons. Ferrier, Hughlings-Jackson, and Priestly Smith, have particularly studied the question of oculo-motor monoplegias and monospasms, that is, of ocular palsies and spasms due to cortical lesions. Dr. Mills believed, with Jackson, that ocular, and indeed all other movements, are represented in the cerebral convolutions. It is necessary, however, carefully to diagnose such cases from those due to lesions at lower levels.

During the life of the patient it was a question whether we had or had not to deal with a case of oculo-motor monoplegia or monospasm from lesion of cortical centres.

Ferrier, in one of his experiments, found that irritation of a certain limited area of the surface of the brain of the monkey caused *elevation of the eyelids, dilatation of the pupils, conjugate deviation of the eyes, and turning of the head to the opposite side*. This area corresponds to a region in the brain of man, at the base of the first frontal, and extending partly into the second frontal convolution. A few cases are on record in which conjugate deviation of the eyes and rotation of the head have occurred without hemiplegia or hemiparesis. Five such cases, or rather supposed cases, have been collected by Ferrier. Some of these were probably, like the case here reported, examples of pontine lesion.

It did not seem probable that the fissured skull, and the small meningeal tumor in connection with it, had any thing to do with the production of the symptoms. The lesion was comparatively remote from the oculo-motor centres of Ferrier, at the bases of the first and second frontal convolutions. Efforts have been made to localize a centre for the levator palpebræ superioris muscle in the angular gyrus, and if such a centre could be made out to exist in this region, it is probable that centres for the other

ocular movements would be in proximity. The weight of evidence, however, both pathological and physiological, is against this localization, and the phenomena in the case under consideration are well accounted for by the pontine lesion. The tumor was also a little too far forward for the angular gyrus proper.

The case seemed to bear out the usual view with reference to the direction of conjugate deviation in pontine lesions. In such cases the deviation is away from the side of the lesion, and toward the side of the paralysis. When the lesion is of the cerebrum, the deviation is toward the side of the lesion, and away from that of the paralysis.

When the question of differential diagnosis is as to whether conjugate deviation of the eyes and rotation of the head are due to pontine or cortical lesion, the following points would seem to favor disease of the pons: The presence, at some stage of the case, of paresis or paralysis on both sides of the body; the existence of disturbances of sensation; contraction of the pupils; depressed farado-contractility; and peculiarities of temperature.

Remarks.

Dr. HAMMOND remarked that he had listened to the paper with a great deal of interest, and he would ask whether the author was familiar with the researches of Landouzy and Grasset.

Dr. MILLS replied that he was.

Dr. HAMMOND said they gave a very different interpretation to such cases.

Dr. SPITZKA, being called upon for an opinion, remarked that he could say nothing in a critical spirit, but with regard to the point just brought up, he doubted whether the cases supported the theory of Grasset. The influence would have to be more or less constant. On the contrary, we found that the ocular movements were not constantly interfered with. As in Dr. Gradle's case, all the influences exerted in the ocular movements by the cerebral hemispheres could be carried on for both eyes by one hemisphere. If the disturbance affected both eyes, there might be a cortical disturbance; but if upon one eye, a cortical lesion was entirely excluded.

Dr. MILLS remarked that he supposed the experiments referred to by Dr. Hammond had reference to the discussion of the question whether it was the first or second convolution of the angular gyrus which was the seat of the oculo-motor centre. He was in-

clined to think Dr. Spitzka's explanation was correct. The one strong practical point from his paper was the fact that we might believe that in conjugate oculo-monoplegia we must make a differential diagnosis.

Dr. MILLS also reported the following case of "Tumor of the motor zone of the brain":

The case was one seen by Dr. Mills, with Dr. F. Dercum, of Philadelphia. The patient, a married woman, aged 32 years, in September, 1878, during an attack of typhoid fever, had a severe convulsion, which left her partially paralyzed in the face and limbs of the left side for four days. In March, 1880, she had a spasmodic seizure, which began with numb sensations in the fingers of the left hand. These sensations were followed by twitchings of the fingers; a spasm soon involved the left arm; and before the attack passed off a general convulsion occurred. After the attack, the left upper extremity was found to be decidedly weaker than the right; subsequently, the patient had half a dozen similar seizures. They nearly always began with twitchings of the fingers of the left hand. The spasm was always most severe upon the left side, was usually limited to it, and was most violent in the arm. When examined early in August, 1880, the left side of the face was partially paralyzed; the left upper extremity was almost completely helpless; and the left lower extremity was paralyzed, but not quite so markedly. Her mind acted slowly. Ophthalmoscopic examination showed double optic neuritis. Hearing was defective in the right ear; she complained of torturing headache, most severe in the right frontoparietal region. Percussion above and around the ear caused greater pain than at any other region of the head. Sensibility was impaired in the left side of the face and left limbs.

She died after great suffering, August 27, 1880.

Post-mortem examination revealed a firm, nodulated tumor, having a mottled appearance on section. It was adherent to the pia mater of the convexity of the right hemisphere, and invaded the middle portion of the ascending parietal and the upper part of the inferior parietal convolutions, pushing aside the interparietal fissure. On the inner side of the tumor, the white matter of the hemisphere was broken down. No other lesion was found, except a slight adhesion of the dura to the pia mater over the upper extremities of the ascending convolutions of the left side. Microscopical examination by Dr. L. B. Hall showed that the growth was probably a carcinoma.

The position of this tumor was accurately diagnosticated during life. The spasm, beginning in the fingers of the left hand, and more marked upon the left side, and particularly in the left arm, pointed to the brachial centres of the motor zone of the cortex of the right side. The left-sided paralysis, greatest in the arm, indicated the same region of the right hemisphere. Impaired sensibility on the left side showed that the parieto-temporal, or sensory zone was probably involved either by extension of the lesion or by pressure. Localized headache, and the results of percussion, confirmed the diagnosis of the situation of the tumor.

"Atrophy of the cerebellum," by Dr. Shaw. The paper upon this subject, owing to its author's absence, was not read.

Dr. F. P. KINNICUTT, of New York, reported a case of "Chorea major," which was chiefly interesting on account of the high temperature, 103° F., and upward, thought to be dependent upon the ceaseless and violent muscular contractions, the improvement under chloral hydrate, and the tolerance of the drug by the patient, a girl of fourteen, who took from seventy to one hundred grains a day.

Remarks.

Dr. MILLS remarked that he believed the author of the paper had referred to the connection of malarial disease with the case, and it suggested itself to his mind, that the symptoms might be due to pigmentary embolism in the capillaries of the brain. He doubted if the persistent elevation of temperature noted was in consequence of the muscular action.

Dr. KINNICUTT knew that such a high temperature was not ordinarily produced by muscular action in chorea, but in his case the muscular actions were so violent that he thought the high temperature was produced by them.

Dr. JEWELL said he had seen two cases, one of which was very similar to the one reported, and in a person of the same age, and in whom it seemed to him it would have been a physical impossibility for the child to have had any more violent muscular contractions than were observed; there was no marked elevation of temperature. The muscular contractions in this instance were so severe that the patient could not be kept upon the bed, except when held by two or three individuals, and at last it was necessary to place her upon the floor. In reference to the administra-

tion of chloral to such patients he thought it should be at the hour of retiring, and in very large doses—what would ordinarily be considered almost toxic.

The next paper was by Dr. E. C. Seguin, of New York, bearing the title of "Aconitia in posterior spinal sclerosis: a new sign of its existence."

I have observed in six well-marked cases of posterior spinal sclerosis, in the first and second stages, a remarkable resistance to the action of aconitia as shown by numbness of the periphery.

These six patients took large doses of the alkaloid, from three to six tablets of $\frac{1}{100}$ grain each in a day, without numbness in the ataxic or neuralgic parts. Numbness showed itself in the parts of the body above the supposed seat of sclerosis, and several of the patients felt faint, dizzy, and quite sick from the medicine.

Dr. W. R. Birdsall, at my request, administered aconitia in full doses to several ataxic patients under his charge with substantially the same effect; one case experienced no tingling, another case had a little numbness in toes, and a third case, after taking four doses of $\frac{1}{100}$ grain, used at intervals of three hours, felt some numbness in ends of fingers; a few hours later was "numb all over."

It appears from these nine cases that tabetic patients are peculiarly insusceptible to the characteristic sensory symptoms of aconitia. This resistance, apparently absolute in some cases, is shown in the first stage of the disease. One of the cases which took at one time $\frac{1}{2}$ (.or) of aconitia in less than forty-eight hours, was examined *post mortem*, and the cord found sclerosed. The aconitia used in these tests was Duquesnel's crystallized aconitia, prepared by Caswell, Hazard & Co., in tablet form. The specific effects of these tablets were obtained during the same period in other cases of disease and in healthy patients. For example, in my own case, $\frac{1}{100}$ grain at 10 A.M. and at 12 noon, made me numb from head to foot, and chilly for nearly five hours.

While not now prepared to advance a theory of the manner in which sclerosis of the posterior columns prevents the sensation of tingling and numbness in tabetic patients charged with aconitia, I feel confidence in my facts, and would offer them as constituting a new negative test or symptom of the disease.

Remarks.

Dr. JEWELL wished to ask a question. The facts recited in the paper of course spoke for themselves up to a certain point, but he

wished to know if Dr. Seguin thought the disease of the sensory apparatus interfered with the action of the remedy.

Dr. SEGUIN replied that he thought so.

Dr. HAMMOND wished to ask if the author's results did not indicate that there were lesions of the gray matter of the cord of more frequent occurrence than was indicated by *post mortem* examination, for sclerosis of the posterior columns of the cord, and involving the lower segments of the cord, could not account for the phenomena in the upper portion of the body unless there were conditions which we could not find, and which this aconitia might show. He thought the experiments showed that there was a lesion the whole length of the cord.

Dr. SPITZKA wished to ask Dr. Seguin what support he had for the claim that the remedy, in acting upon the central gray matter, did not give rise to the peculiar sensory disturbances, because the sensory impressions were interfered with in going outward.

Dr. BIRDSALL remarked that in confirmation of the case reported in the paper, he would say that he had tested the effects of the medicine upon a patient not affected with a disease of this character, in whom the physiological effects of the drug were obtained in the usual time.

Dr. JEWELL remarked that if the remedy acted in the sensorium itself or in the higher parts of the sensory tract, the numbness ought to reach the consciousness of the patient the same as coming from a peripheral nerve, according to a well-known law. Do not such observations teach that either the remedy acted upon the peripheral nerves or upon their points of entrance into the gray matter of the cord, which was the seat of disease in locomotor ataxia? If it acted on more central portions of the nervous system, the impressions would be more subjective and break into the field of consciousness from other regions as well as from the diseased tracts. He asked if these observations did not throw some light upon the question as to what part of the nervous system, comprehensively considered, peripheral or central, was acted upon by the drug. If upon the peripheral, it was not difficult to understand the numbness.

Dr. SEGUIN thought the objections raised and suggestions thrown out by Drs. Spitzka and Jewell might lead to valuable results, which he thought would be in one of two ways: Either there was an unknown lesion in the gray matter in ataxia, and in that gray matter the passage of the abnormal sensation was interfered with; or, second, that the drug did not act upon the gray

matter, but upon the nerve fibres, and as these were diseased, the sensations did not arise.

Dr. SEGUIN then proceeded to read a second paper, entitled "A case of diphtheritic ataxia and paralysis from anal diphtheria—cure."

Mr. B., aged 58 years, has enjoyed good health with exception of hemorrhoids. Never any fulgurating pains, or diplopia.

Nov. 12, 1880, was operated for large hemorrhoids by injection of carbolic acid and oil. Reaction followed, with diphtheritic exudation in hæmorrhoidal masses, chill, febrile movement, and much prostration. Anus well about Thanksgiving (27th).

Early in December seemed fairly well, but a few days before Christmas legs were weak and feet numbish. Gradual increase in weakness of legs, and a few days before examination hands weak, awkward, and numbish. Bladder unaffected; no spinal or peripheral pain, or cincture feeling.

Examined January 25, 1881. Presents paresis of upper and lower extremities, with numbness and slight but distinct anæsthesia of feet, legs, and hands. The striking symptom, however, is the ataxia, which is typical both in hands and legs; no trace of patellar tendon reflex. Pupils normal. During the ensuing two weeks the paresis increased, and gradually obscured the ataxia.

Feb. 5th. Lies quite helpless on couch, almost no voluntary power in arms or legs; sensory symptoms as above. No atrophy or degeneration reaction. Improvement in voluntary power began February 15th, and progressed steadily, with corresponding diminution of the anæsthesia.

March 29th. Walks with a cane.

May 3d. Is practically cured; only remains of attack is a slight occasional numbness in soles of feet; no tendon reflex.

May 17th. A trace of patellar tendon reflex on both sides.

The treatment consisted at first in the use of belladonna and ergot; later nux vomica and iron: At the last a simple solution of strychnia in nitro-muriatic acid, was given.

A thorough electrical treatment and massage were also had. Until March 16th galvanism was used only; stabile ascending current to limbs and spine. After this date faradism was carefully used on the recovering muscles. The massage was made proportionate to the paralysis, and in the last few weeks was vigorously done.

Remarks.

Dr. MILES had seen a most complete case of ataxia about six years previous in a child three or four years old. He had had an attack of diphtheria, and from the first there was paralysis or paresis. The paresis disappeared, but the ataxia remained. Electro-contractility was somewhat diminished. The knee-jerk was not tested, for he was not then familiar with it. The patient recovered in three months under the use of strychnia and faradism.

Dr. SPITZKA remarked that there was one interesting point in a case of the kind reported, and that was the location of the diphtheritic sore.

Dr. SEGUIN said he looked upon the case reported by him as one of myelitis, probably infectious, with deposits of minute organisms around all the anterior and posterior nerve roots entering the spinal cord, probably first in the posterior segments of the cord, and the anterior afterward, judging from the succession of events. He had been much interested in the case because of the difficulty of diagnosis. Dr. Seguin was strengthened in his suspicion of diphtheria from the absence of pupillary symptoms and fulgurating pains.

There being no further discussion, Dr. Jewell moved that the designation of the place and time of holding the next annual meeting be referred to the Council for action. Carried.

The President then declared the Association adjourned *sine die*.